sits of Mercury were observed by La Concha at Monte Video, November 5, 1789; by Keiser at Amsterdam, November 9, 1802; by Fisher at Lisbon, May 5, 1832; and by Houzeau at Brussels, May 8, 1845. Taking for the heliocentric longitudes of the body observed, the tabular longitudes of the earth at the epochs of the observations, the following formula for the heliocentric longitude (ν) at any time, is obtained—

$$\nu = 56^{\circ}{\cdot}04 + 4^{\circ}{\cdot}092307 j - 7^{\circ}{\cdot}66 \sin \nu - 9^{\circ}{\cdot}18 \cos \nu,$$

where j is the number of days from November 5, 1789.

Then admitting the place of the node of the orbit to be in 46°, a transit is indicated by the formula for November 9, 1848, which actually took place.

The problem under discussion, as it refers to a possible intra-Mercurial planet, is susceptible of many solutions, which it becomes necessary to determine. They are comprised in the formula

$$\nu = 139^{\circ}{\cdot}94 + 214^{\circ}{\cdot}18\, k + (10^{\circ}{\cdot}901252 - 1^{\circ}{\cdot}972472\, k) j + (-5^{\circ}{\cdot}3 + 5^{\circ}{\cdot}5\, k) \cos \nu.$$

j in this case being reckoned in days from 1750·0, and k being an indeterminate, which may receive values either positive or negative, but necessarily whole numbers.

If $k = 0$, the solution, very precise, is the one already given where the duration of a revolution is 33·02 days, and the semi-axis 0·201.

If $k = -1$, the solution is as exact as the preceding one. The revolution is 27·96 days, and the semi-axis major 0·180.

If $k = -2$, the solution is less exact; the revolution becomes 24·25 days less than the period of the sun's rotation.

If $k = 1$, a solution of the same degree of precision with the last is obtained, with a revolution of 40·32 days.

And if we put $k = 2$, when the revolution would be 51·75 days, large errors will remain.

In all these hypotheses the calculated epochs of transit in 1859 (Lescarbault) and 1862 (Lummis) are very nearly the same. Under these conditions M. Leverrier assumes that we may venture on the calculation of the times of future conjunctions, which occur in the vicinity of the nodes, situated in 192°·9 and 12°·9, the first point being the ascending node, and with the orbit corresponding to $k = 0$, he determines the times of conjunction in the intervals 1853–1863, 1869–1877, and 1885–1892. The tables show that the epochs of transits will be regulated by a period of about seventeen years, in the middle of which the transits will occur, but after which none would be seen for many years. Lescarbault and Lummis it appears observed at the end of one series of transits, which explains why in searching after them in the same region of the sky observers have not seen anything, and seven or eight years might elapse without more success. M. Leverrier then examines the possibility of a transit of the hypothetical planet in the spring of 1877. The conjunction with the sun would occur on March 22 at a distance of 10°·9 from the node, and if this distance be considered certain, as well as the assumed inclination of 12°, there would not be a transit, but in view of very probable modifications of these numbers, a transit may be possible; and he then urges observers to a close watch upon the sun's disk on the 22nd of March next, seeing that there would be no other transit at the spring node before 1885; and a similar examination of the conjunctions at the opposite node (September and October) shows that for the present they do not occur under more favourable conditions. The conjunction in 1876 would take place on September 21, when a transit, though not altogether impossible, is very doubtful. For a transit at this node it is necessary, under the assumed conditions as to the position of the orbit, to wait until about 1881.

For the present, then, there remains no other resource than a direct search off the sun's disk, and M. Leverrier remarks that Dr. Janssen "ne désespère pas d'y parvenir, grâce aux perfectionnements de l'optique céleste, auxquels il a si puissamment contribué." The remaining part of the communication to the Academy is occupied with ephemerides of differences of right ascension and declination of planet and sun for the last half of October.

Mr. De la Rue has instituted a very close examination of the Kew heliographs, with some interesting results.

THE VARIABLE STARS S CANCRI AND U GEMINORUM.—The following are times of visible geocentric minima of S Cancri, calculated from the elements of Prof. Schönfeld's latest catalogue, where the period is 9d. 11h. 37·75m.:—

	d.	h.	m.		d.	h.	m.
1876, Oct.	30	15	9	1877, Jan.	14	12	3
Nov.	18	14	22	Feb.	2	11	18
Dec.	7	13	35	,,	21	10	35
,,	26	12	48	March	12	9	52
				,,	31	9	9

While the irregularity of intervals between the observed maxima of U Geminorum of late years appears to forbid the hope of making a reliable prediction of these epochs at present, it may assist observation of the right object if it is noted that the variable precedes the principal component of Σ 1158, 1m. 26·5s., and is north of it 7′ 31″. The writer is informed by M. Otto Struve that this star does not quite disappear in the Pulkowa refractor, but with instruments of more ordinary dimensions it is invisible during the greater part of the period of 9½ days. There is a star 12·13m. very near its position.

BIOLOGICAL NOTES

CEPHALISATION.—Such is the name given by Prof. Dana to what he terms a fundamental principle in the development of the system of animal life. Its meaning can be best explained by the employment of the instances used by its author. The lobster and the crab are closely allied decapod crustaceans. In the lobster the tail is large, the cephalo-thorax elongate, and the antennæ of considerable size. In the crab the tail is minute, packed under the cephalo-thorax, which is short, as are the antennæ; and from this we may infer that passing upwards from the Macrural to the Brachyural forms there is an abbreviation and a compacting of structure before and behind the head. "In the whale the tail is the propelling organ and is of enormous power and magnitude, and the brain is very small and is situated far from the head extremity in a great mass of flesh and bone furnished with poor organs of sense." The principle is therefore that in low types "there is, usually, large size and strength behind, an elongation of the whole structure, and a low degree of compactness in the parts before and behind; in the high, there is a relatively shorter and more compacted structure, a more forward distribution of the muscular forces or arrangements, and a better head." The analogy is ingenious, but we can see nothing of value in the argument more than a repetition of the well-known principle that height in the scale of creation and amount of cerebral development are correlated phenomena. Are we to place the koala, which, by the way, is wonderfully like some of the much higher Lemurs in its proportions, at the top of the Marsupial phyllum and the kangaroos at the bottom, because the former wants the tail and has a blunt nose, whilst the latter have an enormous caudal appendage and a slender snout? Is the sun-fish so much higher than the eel, and the ostrich than the lyre bird? We fear that cephalisation is not a true law of nature.

RHINOCEROSES.—Anyone visiting the Zoological Gardens in Regent's Park at the present time can obtain ocular proof of the existence of two species of single-horned rhinoceros, differing in size, texture of integument, and skin-folding. On a former occasion (NATURE, vol. ix. p. 466) we were able to demonstrate to our readers the distinguishing points in the last-mentioned of these features, and in the

present instance we desire to draw their attention to an important paper by Prof. Flower, F.R.S. (*Proc. Zoolog. Soc.* 1876, p. 443), just published, on the differences between the skulls of the same two species. There are thirty skulls of single-horned rhinoceroses in the two great metropolitan zoological museums, and from a comparison of these Mr. Flower has been able to draw several important conclusions. One of these is that in the Indian Rhinoceros (*R. unicornis*) the posterior termination of the bony nostrils (the mesopterygoid fossa) is considerably narrower than in the Javan species (*R. sondaicus*), at the same time that the vomer terminates behind by becoming lost, through fusion, in the pterygoid processes, instead of ending free, lamelliform, and pointed. In the Indian rhinoceros, also, the upper grinding teeth have a pattern which is easily distinguishable from that of the Javan animal, a peculiar little circular "accessory valley" being developed in the first and second molars of the former, not found in the latter. In the same paper Mr. Flower also brings forward an interesting difference between the skulls of the single and double-horned rhinoceroses, the external auditory meatus being embraced below by the fusion of the post-glenoid and post-temporal processes of the squamosal portion of the temporal bone in the one group, whilst in the other these two processes remain separate, as in the horse and tapir. The African species agree with the two-horned Asiatic in this respect, so that the character separates the unicorn from the bicorn Rhinoceroses.

PASSERINE BIRDS.—Within a few pages of the paper above referred to is one by Mr. A. H. Garrod upon some of the peculiarities in the anatomy of Passerine Birds. The nature of the voice-organ is the point laid most stress upon. For a long time it has been known that there is a small section of the Passerine birds which has no muscular organ of voice that may be employed for singing. These *all* were supposed to inhabit America, although from the conformation of their wings, wherein they alone resemble the aberrant genera just mentioned, Herr Cabanis, of Berlin, as long ago as 1846, predicted that the Old World Ant Thrushes (*Pittidæ*), lacked the voice organ. Mr. Garrod, from a dissection of several specimens of two species of *Pitta*, demonstrates that Cabanis was quite correct in his surmise, and that the voice-organ is absent in them. He also describes the same organ in the Lyre Bird of Australia (*Menura superba*), and in its diminutive and interesting ally *Atrichia rufescens*. The paper ends with an outline plan of the classification which introduces more than one novel feature.

BAROMETRIC VARIATIONS

IN the "Notes," NATURE, vol. xiv. p. 464, I see reference is made to my results on this subject, and it is suggested that General Myer's International observations will be of the greatest value in connection with the question whether there may not be some other attractive force than gravitation connected with these variations.

I had come to the conclusion nearly twenty years ago (see British Association Transactions for 1859) that the *mean* pressure of the atmosphere for the whole globe was probably less for July than for January. This conclusion was derived from observations made at a great number of stations in both hemispheres during these months in the same year (1844). A considerable part of the earth's surface was not covered by these stations. About a year ago I received from Gen. Myer a copy of the *Bulletin of International Observations* made on February 7, 1875, at 7h. 35m. A.M., Washington Mean Time, and I was glad to see in such observations the means of making more complete comparisons of the mean barometric pressure for given instants on different days. It was only a few months later that I found I could obtain a sight of other *Bulletins* at the Meteorological Office. I had time, however, to compare only two *Bulletins*, that sent me by Gen. Myer for February 7, and another for the 27th of the same month (1875) which seemed to show a lower pressure generally than the first. Other investigations have prevented me from seeking for a larger series of *Bulletins* to carry out the comparisons; but it seems to me that the comparison then made is sufficiently interesting to merit notice.

The mean barometric pressure at 7h. 35m. A.M., Washington M.T., was found for each of the countries in the *Bulletin*, on each of the two days mentioned; the differences of these mean pressures were then taken; they are given, with the numbers of stations from which the results are obtained, in the following table:—

Country.	Number of Stations.	Difference of Pressures. in.
Russian Empire	23	+ 0·19
Denmark	3	+ 0·21
Greenland, Iceland, and Faröe	3	− 0·48
Norway	3	+ 0·33
Austria	12	+ 0·06
Turkey	5	+ 0·19
Mediterranean, Gibraltar, Corsica	3	+ 0·15
Germany	21	+ 0·30
Switzerland	2	+ 0·68
Italy	13	+ 0·32
Algeria	9	+ 0·27
Netherlands	4	+ 0·52
Belgium	1	+ 0·55
France	21	+ 0·54
Spain	1	+ 0·27
Portugal	1	+ 0·26
Great Britain and Ireland	41	+ 0·32
Canada	18	+ 0·53
United States	96	+ 0·37
West Indies	7	+ 0·09
Ceylon	1	+ 0·21
Cape of Good Hope and Natal	2	0·00

It will be seen that, with the exception of the small area about Iceland, all the differences are positive; or the barometer stood higher on February 7, 1875, at 7h. 35m. A.M. W. M.T., than on the 27th at the same hour. I have no doubt that when the investigation is made with the care it merits, much more marked results will be obtained. All these series, however, with the exception of the last two stations, are in the northern hemisphere; it is then of course possible that the atmosphere was playing at "hide and seek" with us, and had moved away to places for which no observations are at present forthcoming. There may also have been some difference in the amount of vapour in the air on these two days; this I have not attempted to calculate, but for two days in February, in the northern hemisphere, it will probably be very small.[1]

In the first investigation already referred to, I had calculated the mean tension of vapour in the lowest stratum of the atmosphere for each station; this, it is now agreed, does not indicate the pressure of vapour on the barometer, but the result was that the vapour tension was greatest in July, when the mean barometric pressure was least. A reason for the increased mean vapour tension for the whole globe in July will be found in Dove's result that the mean temperature of the whole atmosphere is greatest in that month. I shall probably take the liberty of returning to this subject.

JOHN ALLAN BROUN

[1] I see from the *Bulletin* in my possession (that for February 7) that the thermometer was, on the average, below zero (centigrade) in Europe, and from 10° to 30° below zero in America; the higher pressure on the 7th could scarcely then be due to the vapour in the air. For any considerable exactness in such comparisons, series of observations like those of General Myer should contain the *observed* pressures for each station (or the correction to the sea-level) as well as the calculated sea-level pressures; since if, at any high level station, the observed pressures are *exactly the same* on two days, one of which has a higher temperature than the other, the calculated pressures for the sea-level will differ, that for the lower temperature being highest. The greatest mean error due to this cause in the present instance will not, in all probability, exceed ±0·01 inch.